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PROFESSOR DONALD WILLIAMS VERSUS HUME

WHY can't we settle anything in philosophy? Even when the clear, final, inescapable facts have been long ago thrust upon our attention?

Hume pointed out that from past and present data we can not *deduce* any prophecy of the future; not even a merely probable prophecy. Professor Donald Williams in his brilliant book, *The Ground of Induction*,[1] maintains that we can; that from uniform data in experience we can prove a probability that the uniformity will continue. And this on strictly deductive principles.

Professor Williams, in spite of all his care and power of reasoning, seems not fully to have noted how deep Hume's argument cuts. We can not infer deductively from the existence of one thing the existence (even only probable) of another or quality. The though may be thrown into this form: we can not deduce one particular from another—no matter how extensive our first particular may be—even if it consists of a multitude of particulars. Such a deduction would have to be an "immediate inference," an inference with but one premise, the conclusion of which was a proposition about something not mentioned in the premise! The sanction of logic has been claimed for many a monstrosity, but it will hardly be claimed for this. Hume's thesis therefore stands. And Professor Williams's is inconsistent with it.

It is inconsistent because he starts with particulars, invokes no factual generalization that could serve as major premise, and ends with particulars (as well as generalization)—in a probable form. He does not oppose Hume's argument in direct collision, but he would seem to find his way round it—by a course of reasoning of a captivating ingenuity. From anyone interested in all attempts to confute Hume on this topic the book can not fail to command signal attention.

For the sake of simplicity and clearness in a subject so easily thrown into complexity, I may be permitted to approach the author's argument by steps or stages.

I

The commonest reply to Hume is this: we know that if the same event occurs again under precisely the same circumstances it must

[1] Cambridge: Harvard University Press. 1947. ix + 213 pp.

produce the same: if its nature is identical how can it produce something different? This argument seemed cogent, for example, to both Bradley and Bosanquet. Yet it is a mere trick with words. To produce is to cause; you are saying that the event was a cause, and by the definition of the word "cause" or "produce" the same cause or productive agency must always have the same effect. But how do you know that the event *was* a cause, that it has a right to the name, that it always will be followed by the same sequel? That is an implication of the term, not of the event. Hume pointed out that what we call the cause and what we call the effect are two distinguishable events, that we can think of each by itself, that is, apart from the other, that the one, taken simply as an event, does not imply the other, that it is only from experience we learn that the one may be expected to usher in the other. The verbal trick begs the whole question. From this evasive fallacy Professor Williams's book is wholly free.

II

The species of argument employed in the book is the mathematical calculation of probabilities. Let us begin, as an introduction, merely to illustrate the conditions of any such calculation and without any reference whatever to the book's argument, with some of the simplest examples that Professor Williams happens quite incidentally to mention.

> If, for example, the conditions of a race (p) entail that just one of six horses must win ($q_1, q_2 \ldots q_6$), and if there is no other relevant information, then the credibility of any one horse winning is $\frac{1}{6}$. [Pp. 34–35.]

Over the "logic of chance" or "theory of probability" a great illusion has for many minds long hovered: the illusion that pure deductive reason can extract a probability solely from "the possibilities of the case." Out of this has grown the dream that we can found induction and natural science upon such a procedure of pure deductive reason. But you can not extract a probability from mere possibilities. The idea of probability is not contained in the idea of possibility or possibilities and can not be conjured out of it.

In the example just given there is specified a definite fact (not a mere possibility) which is the foundation of all that follows, namely, that a certain event will take place: the race will be run. In real life, if a race is announced and expected, this is a high probability arising from experience; in a hypothetical case like the present it is part of the hypothesis. But this event that is going to happen may take any one of several forms, that is, one or another of the horses may win. Each of these forms of the outcome is

rightly called possible, but they are simply possible alternative forms of something which is not merely possible but certain or highly probable. This certainty or high probability they divide up between them. Thus we are *not* dealing with mere possibilities but with probabilities each of a low degree. In a race in real life each of these low probabilities (that a particular one of the horses will win) is $\frac{1}{6}$ of a high probability (namely, that some horse will win); in a hypothetical case each is $\frac{1}{6}$ of a hypothetically imagined certainty. If we did not have our probability or assumed certainty at the outset—that the race would be run and one of the horses win—we should have no emergent probability at all. And this applies to every case of a mathematically computed likelihood of this order. We might reason forever about mere possibilities and arrive at no fact or probable fact.

And there is another requisite, of course, besides mere possible cases, namely, the limitation of their number; in this case the known fact that *only* six horses will compete. That is not a possibility but an ascertained or definitely assumed fact, without which the inference would be impossible.

Look at another of Mr. Williams's incidental examples.

> . . . to aver that the premise that a bag of marbles contains 9,999 white and 1 black gives no rational support to the belief that a marble abstracted from it will be white, is strictly absurd.

Certainly; we have no reason, when we take out one, to expect that any particular marble more than another will be the one taken. Hence we must be prepared to the degree of $\frac{999}{1000}$ to see one-or-another-of-the-white-marbles, and be prepared to the degree of $\frac{1}{1000}$ for the black.

Here the event that it is assumed will take place is the drawing of a marble from the bag—the revelation of the color of some one of the marbles, like the revelation of the comparative speed of one of the horses. There are one thousand forms, so to speak, in which this may take place, since there are one thousand different marbles each of which may be the one drawn. The probability that any particular marble will be drawn is a thousandth part of the assumed certainty that the event will occur.

So let us formally set down the following as the first and second of certain principles governing our subject.

1. *Calculation under the Theory of Probability is based on an initial certainty or probability that something exists or will exist; there is, however, more than one form or embodiment in which it may exist, and the number of alternative forms or embodiments is known.*

2. *Probability can never be extracted from mere possibilities, the former idea not being contained in the latter.*

Suppose now we try as an experiment to reason inductively, that is, from experience, as nearly as possible after the fashion of these two examples. *I am not connecting this with any procedure of Professor Williams's.* I try it as a preliminary for the light it sheds on the whole subject. Suppose, for instance, having always found that "water wets and fire burns," we ask: On the occasion when next we encounter fire or water how shall we reckon on deductive principles the probability that it will burn or wet? That occasion would be intended to correspond with the race and with the discovery of the color of a particular marble. At once we see that there is no parallelism. Deductively, we do not know that there will be any such occasion. Deductively, we know nothing at all about any future events. We expect them, owing to our experience, but not on deductive grounds. We have no information from that source limiting the number of forms in which our event may occur, or showing us that objects will retain the properties they now have. Hence the entire basis of procedure in the two examples is lacking and no ground remains for deductive procedure at all.

III

We come now to Professor Williams's own manner of proceeding in offering a basis for induction. It consists in passing from the calculation of what a sample will probably be when we know something about the whole class to the calculation of what the whole class will probably be when we know a sample. It is the latter that induction requires—the sample being our data of a certain kind or class up to the present. And the misfortune of the argument is that we can not effect such a passage; that which enables us to make the calculation in the first instance is the very thing that is completely absent in the second; we have nothing from which to draw a conclusion. In the first case at the outset we have a knowledge of the whole class and to that extent a knowledge of the sample, which is part of it. In the second case we have a knowledge of the sample and none whatever of the rest of the class.

I am not, of course, seeking dogmatically to exclude Professor Williams's view in advance; I am saying that this is how the matter appears when we look it deliberately in the face. If there is any logic that will deliver us from this apparent *impasse* we shall certainly salute it as a marvelous discovery.

If we try to extricate ourselves from our ignorance of the rest of the class by considering *all possible* members of it, and what alone they *could* be or must be, then we encounter at once two broad considerations: (1) we have no deductive means of knowing anything whatsoever about what the rest of the class could, must, or will be; and (2) if we had we still should not know deductively that any more of the class existed or would exist. We have no deductive knowledge that the universe or any part of it will continue to exist beyond this moment.

Of course we are constantly inferring with regard to the future from uniform facts already in hand; what Hume said was this is another kind of inference; it is not deduction.

It seems entirely clear that if we are going to draw deductive conclusions of any sort about the nature of the rest of the class—what properties further things of that kind will turn out to have—we must begin by making some sheer factual assumption about it. We can not draw conclusions from nothing. We can not distil them from utter ignorance. But, granted an arbitrary assumption of one sort or another, we can proceed to base conclusions upon it. Now to make an assumption which in fact is arbitrary about the actual composition of a class, in advance of our knowledge of any part of it but our data, and then to base deductions upon this arbitrary assumption, is, I think, exactly what we shall presently find Professor Williams doing. We have our sample, we have nothing more; in some manner an element of fact about the unknown remainder must be imported into the matter if we are to seem to reach any probability.

The appeal of his book is to "the fundamental law of large numbers, that most samples [for instance our data] match their populations [the membership of their class]." ". . . this is an analytic law of logic" (p. 153). But where does he argue the matter, where do we find the nerve of his actual proof of this law as a basis for induction, what is the piece of reasoning which has the responsibility upon it of being the one means of escape from Hume's conclusion? We are manifestly intended to find it in the chapter on "The Probability of Induction." There on page 91 we read: "Here, I take it, is in main principle a solution of the problem of induction." What has preceded? An attempt, as already said, to pass from the calculation of what a sample will be when we know something about the whole class to a calculation of what the whole class will probably be when we know a sample; to calculate the latter in a comparable manner *and with comparable warrant* (the thing that I suggest is impossible).

> What we have to work with in an inductive situation consists of two kinds of knowledge: the number of the sample, and its composition, which are facts of observation; and the principle that no matter what the population is like, any sample, of considerable size, will very probably be very similar to it, this being an *a priori* law of classes, demonstrable by logical analysis. [P. 91.]

What, then, are the steps of the argument?

> In the first place, although the inferring of the composition of the population from the composition of the sample is not logically the same as the converse process of inferring the composition of the sample from the composition of the population, both inferences could be made valid by the single general principle that given any population and any (largish) sample thereof, it is highly probable that the sample matches the population (and hence that the population matches the sample). In the second place, the latter general principle may be proved to be true by no more intricate method than considering *the whole range of possible population compositions* and observing that although the probability of a matching sample of a stipulated size is different for different compositions, the probability of a matching sample is always greater than the probability of any other kind of sample, and if the sample is large, then *no matter what the population composition may be*, the probability is very great. [P. 88.]

"*To bring this out clearly*," he says—the emphasis is my own—"let us look again at the sampling formula and ask how much we can say about its various terms even though we do not yet know the actual composition of the population." He points out what "the extreme cases" would be: if *all* the class had the character in question, all the class would in that respect resemble any sample; if none of the class had the character in question again all the class would in that respect resemble any sample. True.

> On the other hand, the probability of matching is least, and the probable deviation σ is the greatest, when the composition of the population is midway between the two extremes 0 and 1, that is, when just half of M is P and half accordingly is non-P. . . . What happens then in this worst of all conceivable situations for the probability of the sample's matching the population? . . . *the worst possible situation is still a very favorable one.* That is, there must be a probability of .6826 at the very least that the sample composition does not differ from the population composition by more than 1 percent., and a probability, at the very least, of .9545 that it does not differ by more than twice this, or 2 percent., and so forth." [P. 89.]

This seems a strangely artificial approach to the question, How do we know that the data we have are any guide to the future? Our situation in induction is not that of contemplating academically a merely conceived class and considering upon what hypotheses as to its composition any possible sample will be likeliest or least likely to be in a certain respect a true specimen of the whole class. Our situation is that of having an actual specimen already in hand (our data) and desiring to know what prospect there is

that the rest of the class will in the respect in question be like it. And "the probability of matching is least" if *none* of the rest are like it. *That* is "the worst possible situation." To say that at least half the class must resemble the sample is to make an arbitrary factual assumption about the rest of the class; it is just here that we find the author doing this. Inasmuch as in the cases that arise in induction the possible other members of the class are for us wholly uncertain in existence and, if existent or so to be, wholly unknown in character beyond being members of the class, we have no assurance, not even any degree of probability, in the matter. I speak, of course, from the strictly deductive point of view, which alone is here in question. From that point of view our data are no clue to anything. Which is just what Hume pointed out.

To be sure, the assumption that one-half of the class have the character in question is midway between assuming that all have it and that none have it. That none have it we know to be false because our data have it. That all have it we possess, of course, no deductive means of knowing. But he has shown us nothing that obliges us, from our actual point of view, to deem the midmost situation between these particular extremes the worst possible. He has shown us nothing that obliges us to make any assumption at all.

When the author invokes "the law of large numbers" he must not forget that its substantiation as supporting induction has been effected *through* the reasoning just quoted, and apparently depends upon that reasoning. That seems to be the bottleneck of the argument. If the law can be substantiated as supporting induction apart from that reasoning, he must show us just how.

It does seem to be this very argument just cited that is offered as "in main principle a solution of the problem of induction." True, he mentions at least one other "way of formulating the solution" (p. 93), but it is briefly and dogmatically stated, without rendering clear the one essential, the nerve of proof. What we need to know is the innermost logic of his solution, explicitly, completely stated, and if we do not find this in the passage quoted above, where do we find it? If I have missed the pith of his argument my only desire is that my eyes shall be opened and that I may see it set palpably before us. I am pathetically ready to be convinced, to recant, and to become an advocate of his thesis. If he has supplied a logical confutation of Hume he has given us the most momentous production on the subject since that philosopher wrote. Attempts by others at confutation up to the present have signally failed. But it would seem that Professor Williams has a liability to be carried away by the technics of symbolic

logic out of sight of first principles and first needs. These technics have been and are, of course, invaluable. But it is a healthful exercise, every little while, to translate them into English, and had he done so he would probably have avoided some of the reasonings presented in this book.

Needed: a formulation of the law of large numbers as basis for induction which does not require any arbitrary assumption about the unknown. Needed for the author's position but apparently impossible. If possible it is, might it not be stated in full, without dependence on allusions, taking nothing for granted?

In the absence of such a formulation may we not now say:

3. *There is no means possible or conceivable by which from given specimens of a class could be deduced the probable character of its other members. For the reason that in the nature of any existent or existents no implication is present as to the nature of any other. From concrete particulars we may gather empirical probability, yes, but implication, no.*

4. *While the deductive inference of probabilities depends always upon definite knowledge or assumption of an antecedent probability or certainty which it can not transcend, the inductive inference of probabilities, on the other hand, depends upon data which it is its very function and essence to transcend.*

This is in effect what Hume laid down.

IV

We turn now to the philosophic background of the work.

In drawing an inductive inference from our data to further particulars, if we could supply a major premise, such as the Law of Causation or "the Uniformity of Nature," and certify it apart from experience, our inference would become deductively valid. But we can not. And Professor Williams offers at the outset no principle respecting the sequence of events in nature. Any such general proposition would be "synthetic," and he maintains that no such is required. "Induction presupposes nothing."[2] He does invoke "a principle," "the law of large numbers," but it is "an analytic law of logic" (p. 153), "an analytic truism" (p. 139), "an *a priori* law of classes, demonstrable by logical analysis" (p. 81). "Since the principle is purely logical, presupposing nothing about its data or its objects except that they be members of classes —a status that no conceivable entities could avoid—it makes induction quite independent of any supposed maxim of causation, uniformity, or sufficient reason" (p. 202).

[2] A chapter-heading.

If the nature of our data in induction involved membership in a class that must include other members, *existent or to be existent,* than the data themselves, then their nature would prove the existence of other particulars, and Hume would be wrong. Or if their nature involved that the possible other members of the class would probably if they existed have a further quality like that of the data, besides belonging to the class, then again their nature would prove a superadded fact respecting existence, though a conditional fact and a fact of probability alone; their implication would stretch out into the world of reality beyond themselves. But plainly neither of these suppositions is or could be true. It is other *possible or conceivable* members and qualities only that the class must contain to which they belong. And to this Hume could have no objection, for it does not contravene his thought.

Yet it is the idea that they are members of a class from which the author derives his whole theory of the basis of induction. The class-idea is the hat out of which he gets the rabbit.

The broadest comment to be made on Professor Williams's philosophy of induction is the following: if the only element you introduce into the reasoning besides your data is an explicative or "analytic" proposition, it can never enable you to infer any existent or probable existent, even if it be only a quality. If your major premise had said, "This kind of fact is always coupled with a certain other" (a synthetic proposition), then you could have concluded, I repeat, to further fact—a real induction. But if it only says, "This kind of fact always contains a constituent attribute of this kind of fact" (an analytic proposition), you are not enabled to go further; you are left with your data and nothing else on your hands. Upon such a major premise induction can never be founded. What is apparently deemed the strength of the argument, its economy of philosophical assumption and immunity in that respect from scepticism, consisting in its having dispensed with all but analytic, that is, self-evident foundations, turns out to be its weakness. This is simply one aspect of a truth that we may submit as a fifth among the principles that the book obliges us to recall.

5. *We can not prove from the definition of terms, no matter what they are, nor from any analytic proposition, that something will exist or probably exist; nor can we even when the proposition is taken in conjunction with concrete data.*

(If the principle and warrant of our empirical knowledge, the saving and governing knowledge of our life, arose out of the depths

of "identical" and merely verbal propositions, what a singular logic would be ours!)

Coming a step closer to the specific argument of the book we may add, therefore,

6. *No law of numbers, no mathematico-logical laws, can prove the existence or probable existence of particular units in nature or particular qualities of such units.*

As Hume would put it, we can not in inference pass from "relations of ideas" to "matters of fact." In other words, we can not infer from the relations of implication amongst abstract terms of thought that anything concrete exists. *Even granted that certain units* (*our data*) *exist,* we can not deduce from such relations that other units exist—either certainly or probably. Terms imply according to their definition. There are two cases possible. Either our term implies another existent, as "husband" implies a wife, in which case we must have separate and empirical evidence of the other existent before we know that the term is applicable; or it does not imply another existent, in which case it can not be used to prove that there is any other. In neither case does our datum *plus* a definition or *plus* any analytic proposition prove the probable existence of anything else, nor even a probable existent quality of it, in case the thing itself does exist.

And when we watch Professor Williams at work basing induction on the law of large numbers we find him obliged, as we have seen, to make a factual assumption about the whole class with which he is dealing, namely, that at worst one-half are like the sample, which assumption is not analytic and not a truism and not deducible from any truism.

V

In conclusion may be mentioned Mr. Williams's treatment of logical implication as if it had degrees. He speaks of "a degree of implicative connection intermediate between strict entailment and inconsistency" (p. 47). Again, "a non-demonstrative logical implication" (p. 134). Again, "a relation analogous to entailment, albeit of an intermediate degree" (p. 46). Etc.

Surely there can not be such a thing as a degree of implication. A degree of probability—yes; but not a degree of implication. Implication is entire and absolute or it is not what is meant by the term. If the conclusion does not strictly follow from the premises then it does not follow at all. To follow means to be true if the premises are true. It may be said that in some cases it is only

probable if the premises are true. But then the probability does follow, and unreservedly. A probability may be implied as truly as a certainty; all depends on the content of the premises.[3] But in that case the conclusion is certainly true if the premises are, the conclusion being that a probability obtains.

He formulates "the proportional or statistical syllogism" thus: if m/n M is P, and a is M, then there is a probability of m/n that a is P. And he tends to treat it as in a sense or degree deficient or falling short, in that it issues in a probability only, which accrues, as a substitute that we must put up with, in place of a conclusion that is certain.

The shortcoming, however, is not in the probative force of the syllogism but in the knowledge with which it sets out. Granted that knowledge, granted that we understand the premises in their completeness, the logical force of the argument is in no respect short of absolute implication and coercion. Once more, the conclusion as conclusion from those premises is certain and not merely probable, though it is only the assertion of a probability. And better than certain the inference could not be. Probability is not a growth that germinates in the cracks and gaps of a deficient syllogism. I am not offering objection to any artifices of procedure that prove convenient for mathematical logic. What I am saying is that it is only by a confusion of thought that we can assert degrees of implication.

Mr. Williams writes: "What we require is an 'ampliative' inference (to use Peirce's phrase), proceeding in accordance with abstract principles whose validity is evident or demonstrable in the same way as the rule of the syllogism, from factual premises . . . to something *further*" (p. 11).

But if it passes to something further, if it passes beyond the exact contents of its premises, it is not deduction. If it does not pass beyond it is not "ampliative." When is deduction not deduction? When it is "ampliative." It is the whole essence, warrant, claim, and soul of deduction to be explicative. What Mr. Williams would have to do, what fundamentally he is trying to do, is to seek a rational support for induction by an explicative process. (This term is used just here, of course, like Peirce's term, in a wider sense than the established one, namely, as the opposite of the latter.)

So the last of our relevant principles is:

[3] Mr. Williams explicitly discusses this matter just as he explicitly discusses Hume and just as he discusses or approaches other matters of principle put forward here. In this brief article I can not attempt to enter into all the indentations of his coastline, but I steer a course which I hope subtends them.

7. There are no intermediate degrees between following from premises and not following from them. There is no such thing as half-following or quarter-following.

This is not, of course, a review of the book. Here I am concerned with but one thing, the thread of proof that runs through it. There is much in the book that awakens high admiration. But it does leave a deep regret that rare capacities and knowledge should have been devoted to an enterprise that Hume exposed as impossible two hundred years ago: the laying of a deductive foundation for induction. The more plausible such an attempt is made, the more it interferes with the advance of philosophy, which is wholly dependent on the recognition of distinctions once they are discovered.

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COMMENTS AND CRITICISM

A NOTE ON "INSIGHT"

Donald Williams in his book *The Ground of Induction*[1] cautions his readers against the reliance upon "insight" as a ground of truth in that it is a recourse to mysticism and dogmatism and fanaticism. There is no such faculty, he says, and to claim that there is is usually the warrant for the justification of some presupposed dogma. And yet throughout his book he mentions innumerable times the fact of man's native logical wit. What is this native logical wit if not the "insight" he warns against?

Those of us who adhere to the scientific method and to the logic of science, and to Donald Williams they are the same, to which we agree, need not look upon the power of "insight" of man as a mystical faculty, nor as a non-existent one. However, we are still at the stage where we must admit that "insight" is still mysterious, or still unexplained. It operates at every moment of human life where the creative response to stimuli is made necessary by the novelties of experience. Where life is automatic and reflexive and habitual there is no need of insight. Where people are themselves without insight they rely upon the insights of others or the accumulated and codified insights of tradition and history and custom.

At the lowest level of the operation of insight—a child learning to walk, let us say—we find the inexperienced human mind relying completely upon this faculty. And at the highest level—Donald

[1] Harvard University Press, 1947.

Williams reflecting upon his theory of "dialectical induction," let us say—insight is operative. It is still at the present stage of our knowledge the ungraspable principle of the measurable graspable principles that constitute knowledge and logic.

Whereas Hume could not verify the existence of this human faculty and succumbed to his scepticism, it is not necessary for us to do so. Hume failed because he did not believe in truth. Those of us who believe in the attainment of truth start out with the advantage of being able to believe in a method or methods of attaining it. It follows necessarily that we have faith in man's native ability to construct nets for the fishing and catching of truth. It is this native ability that is the lair of that mysterious, but not mystical, faculty called "insight."

In claiming the existence of "insight" it does not mean that we must challenge the validity of "dialectical induction." As a method of testing "insight" it is a remarkably reliable one. As a check upon insights that may claim to be true but are false it is again most reliable. Indeed, if we were to forego the use of "dialectical induction" we would be making "insight" into a mystical faculty.

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BOOK REVIEW

The Ground of Induction. DONALD WILLIAMS. Cambridge, Mass.: Harvard University Press. 1947. ix + 213 pp. $3.00.

The hope of supplying a "rational justification" for inductive inferences in general, where induction is defined as any inference from observed cases to unexamined ones, continues to leaven philosophic reflection. Since Hume's critique of classic rationalism such justification has been attempted in various ways: on the basis of theological assumptions, of assumptions concerning the constitution of the cosmos, of analyses of causality, of theories dealing with the structure and faculties of the human mind, of considerations drawn from analyses of probability. However, proposed demonstrations of the necessary validity of induction simply beg the problem at issue if universal premises concerning matters of fact are assumed in the alleged proofs. Many thinkers, past as well as contemporary, who agree with Hume that no propositions about matters of fact are logically necessary and who also maintain that such propositions are unavoidable as premises in any attempt to establish the worth of induction, have therefore concluded that an *a priori* justification of inductive inferences is im-

possible. To such thinkers there is a significant general problem of induction, only if the problem is understood as one calling for an adequate descriptive analysis of the methods actually employed in the successful pursuit of factual knowledge.

Professor Williams's book reopens these much debated issues, and reaffirms the belief that the ground of induction is more than "irrational belief" or "pragmatic postulation." It is in effect a vigorous protest against what he regards as the "scepticism" concerning the powers of reason displayed by current positivistic and pragmatic doctrines. The central thesis of his book is that the validity of ampliative inferences can be certified by purely analytical, *a priori* means, without using a single assumption concerning contingent matters of fact. More particularly, he claims to be able to demonstrate rigorously, simply by considering purely formal relations between propositions, that "knowledge that in an observed part of a class M the property P is present in a certain proportion gives us a *reason*, analogous to that provided by the premises of a syllogism though less conclusive, for believing that in the whole of the class M the property P is present in a similar proportion" (p. 20).

The substance of the book is devoted to an attempted proof of this heroic thesis; the remainder is concerned with a comparison and criticism of rival analyses of induction, and with the application of Mr. Williams's chief conclusion to the logic of science and to several standard problems of philosophy. Although much that is said in connection with these matters is arresting, it is with Mr. Williams's central contention that the present review will be occupied. His argument falls into two parts, the first dealing with the nature of probability, and the second with the probability of one type of ampliative inference. It is with the first question that any appraisal of his argument must therefore begin.

Mr. Williams's views on probability and his attempted *a priori* justification of induction are not novel. He explicitly recognizes that they are only variants of the classical Laplacian account of probability, and he also acknowledges his indebtedness to certain aspects of Peirce's thought. Moreover, he follows Keynes in regarding probability as a logical relation between propositions, though he rejects as "mystical" the latter's contention that it is an unanalyzable and generally non-quantizable relation. But while laying no claim to novelty, Mr. Williams does claim that his presentation of familiar doctrines is clearer and more persuasive than are the more customary expositions, and that his formulations have successfully outflanked difficulties which beset these latter.

To come to details, Mr. Williams's account of probability is

briefly as follows. Traditional formal logic recognizes just two "credibility relations" between propositions: the relation of "certainty" (when a proposition p entails another q), and the relation of "contracertainty" (when p precludes q). There are, however, intermediate cases of credibility, of the same *kind* in respect to the "logical necessity and objectivity" of the bond between premise and conclusion which traditional logic notes, though different in *degree*. The typical and fundamental variety of such intermediate relations of credibility (or probability) is taken to be the statistical syllogism: if a proportion m/n of a class M has a property P, and a is a member of M, then, with a degree of probability m/n, a is P. The relation between premises and conclusion of such a syllogism is held to be a *logical* relation, since the inference from premises to conclusion derives its cogency from the *logically necessary* fact that inferences of this form having true antecedents must have just m/n true consequents—with the obvious proviso that only distinct propositions are counted in arriving at the proportion. (In this summary exposition the class M is assumed to be finite; Mr. Williams also briefly considers the case when M is allegedly infinite.) Thus, just as in the categorical syllogism: All dogs in Boston are biters and Skipper is a Boston dog, necessitates the conclusion that Skipper is a biter; so in the statistical syllogism: .75 of the dogs in Boston are biters and Skipper is a Boston dog, lends a probability of .75 to the conclusion that Skipper is a biter. The sole difference between the two inferences is that while in the former the conclusions are *always* true when the premises are true (where all possible minor premises referring to different individuals are considered), in the latter only .75 of the conclusions are true under the same hypothesis.

It is evident from this summary that Mr. Williams has adopted the familiar Laplacian mode of specifying probabilities in terms of the ratios of sets of exhaustive and exclusive possibilities or alternatives, couched in the language of Peirce's "truth-frequency" analysis of probable inferences—with the one chief difference that he has dispensed with the Laplacian requirement that the alternatives must be "equiprobable." Accordingly, given the statistical composition of a class of elements with respect to their possessing an indicated property, the degree of probability with which the premise "necessitates" the conclusion that some specified member of the class has that property, is a question which pure logic is competent to decide. No empirical information other than that supplied by the statistical premise is required for this task. Thus, given that half the faces of a cubical die are marked with an odd number of points and that a is one of the

faces, it follows with a probability of ½ that *a* is so marked; and no experiments, such as repeated tosses of the die, are required for establishing this probability. Mr. Williams is thus obviously correct in claiming that probability as he has defined it is a gradated *logical relation* between propositions.

None the less, his elimination of the Laplacian requirement that the alternatives entering into the definition must be equiprobable, raises at least one serious difficulty for himself, however much it enables him to ignore others as irrelevant: for in consequence of the elimination of this requirement, premises that are logically equivalent can be shown to stand to the same conclusion in *different* degrees of probability. For example, if a bag contains just two marbles, one red and one white, then according to Mr. Williams the probability is ½ that *a* (a specified marble in the bag) is red. But the premise of this inference is logically equivalent to the assumption that the bag contains one white marble and a red one which either has a diameter of an inch or a diameter different from an inch. And on this latter formulation of the premise, the exhaustive and exclusive alternatives for any specified marble *a* in the bag are: it is red with an inch diameter, it is red with a diameter different from an inch, and it is white—so that the probability that *a* is red is now ⅔. Mr. Williams is not unaware of this uncomfortable outcome, although he diagnoses it as being simply one of the "growing pains of a subtle and lively subject" which he handsomely bequeaths to others to eliminate (p. 162). Some of his readers will be unable to agree with him that this difficulty presents no special obstacle to his proposed emendation of the Laplacian definition of probability. However, no reader can rightly deny that Mr. Williams is on firm ground when he maintains that, on his definition, probability is a relation having the same "logical necessity and objectivity" that belongs to the more familiar relations treated by traditional formal logic.

But Mr. Williams enters upon less firm territory when he proceeds to ground the "reasonableness" of expectations concerning matters of fact in his relations of probability. For he is compelled to face the crucial question whether the credibility relations he has specified can justly serve as the basis for warranted prediction and responsible action—whether, in particular, "a steadfast adherence to probable propositions . . . infallibly in the long run result[s] in an amount of profit proportionate to the probability." And he goes on to explain that "although we cannot be sure that in believing any one merely probable proposition we must be right or even partly right, we ought to be sure

that by persistently believing in highly probable propositions we shall be right more frequently than if we believed improbable ones" (p. 59). This is the issue he discusses under the heading "The Maturity of Chances," and its meaning will perhaps be grasped most quickly if the question is put in terms of one of Mr. Williams's own illustrations. Suppose that there are 1000 bags each containing 900 red marbles and 100 white marbles, so that the probability of any specified marble in any one of the bags being red is .9; and suppose that one marble is to be extracted from each of the 1000 bags. How many red marbles will appear in the set of 1000 marbles thus obtained? The problem therefore is this: given the statistical composition of each of 1000 bags (that is, given the probabilities of red marbles in these bags), have we any rational ground for expecting that these probabilities will "mature" in the 1000 marbles actually extracted from the bags?

Mr. Williams's answer is unambiguously in the affirmative. For while he is careful to note that it does *not* follow demonstratively from the premises that .9 of the marbles extracted will be red, he does maintain that the premises *logically necessitate with a "high probability"* the conclusion that "the actual proportion of successes will be approximately the same as the single probabilities, namely .9," the degree of this high probability being greater than .99. Though the example is trivial, it is nevertheless typical of the uses to which information about probabilities is commonly put. Mr. Williams's general conclusion therefore is that it is highly probable that chances do mature; and the ground he offers for this high probability is the analytical necessity of the statistical syllogism and of the "logical law of large numbers" (pp. 60–61).

But though Mr. Williams is confident he has produced a purely logical proof that chances mature with a high probability, his demonstration is in fact incomplete without the introduction of a special *empirical assumption*, so that his argument as it stands is demonstrably fallacious. It must be noted, in the first place, that when the proposition that a specified marble in a bag is red is said to be necessitated with a probability of .9 by some premise concerning the statistical composition of the bag, all that this means on Mr. Williams's account of probability is that .9 of the marbles *in the bag* are red. The assertion means nothing else. In particular, the degree of probability mentioned in it supplies no information whatsoever concerning the approximate ratio with which red marbles may be *extracted* from the bag—unless a further, most crucial assumption is added that the ratio of red marbles *extracted* is approximately the same as the ratio of red marbles *in*

the bag. That this assumption is not a logically necessary one, is perhaps too patent to require explicit argument. Nor does it follow necessarily from the premise that .9 of the marbles in the bag are red, as is evident if one recalls Mr. Williams's explanation of the cogency of the statistical syllogism. It *is* a necessary truth, upon which Mr. Williams rightly insists, that in syllogisms of the form: "If .9 of the marbles in the bag are red and *a* is a marble in the bag, then *a* is red" (where "*a*" designates distinct marbles) with true antecedents, the consequents are true with a relative frequency of .9. But it is just an error to maintain as a necessary truth that in inferences of the type: "If .9 of the marbles *in the bag* are red and *a* is a marble extracted from the bag, then *a* is red" having true antecedents, the consequents are true in the ratio of .9. Accordingly, while the assumption in question *may* in fact be true and frequently appears to be true, it can not be accepted without support from *independent* empirical evidence. But without that assumption Mr. Williams's demonstration does not hold water; with it, he has failed to establish by means of pure logic alone that with a high probability chances do mature.

Mr. Williams's appeal to the "logical law of large numbers" or Bernoulli's theorem to support his case involves an identical non-sequitur. For on his definition of probability, this famous theorem simply states a fact of arithmetic: it specifies the proportion of combinations satisfying prescribed statistical conditions, in the set of all possible combinations of equal size that can be formed out of the elements belonging to some class with a given statistical composition. Thus, reverting to the earlier example of 1000 bags each with 900 red and 100 white marbles, the theorem asserts the following. Consider the set *M* of all possible combinations with 1000 marbles in each, where each combination contains just one representative from each of the 1000 bags. The number of such combinations in *M* is 1000 raised to the thousandth power. Of these, a certain proportion contain, say, anywhere from 850 to 950 red marbles, and a familiar calculation shows this proportion to be greater than .99. Accordingly, the ratio of combinations in *M* whose statistical composition is approximately the same as the composition of any one of the 1000 bags (the approximation is good within 5%), is close to 1; and using Mr. Williams's definition of probability, it can be asserted as a necessary truth that under the asumptions given, there is a probability close to 1 that any specified member of *M* contains approximately .9 red marbles. However, Mr. Williams's conclusion is that there is a high probability that the chances *mature*—that is, that if one marble is *extracted* from each of the 1000 bags, the probability is greater than .99

that approximately .9 of the 1000 marbles thus obtained will be red. But this conclusion in no way follows from Bernoulli's theorem. For while that theorem does specify the probability with which a combination belonging to M contains approximately 900 red marbles, it yields no information whatever concerning the proportion of combinations satisfying this statistical condition that may be *actually selected* from the 1000 bags—unless, once more, the independent factual assumption is introduced that the ratio in which such combinations are *actually selected* is the same as the ratio of such combinations in the set M of all *logically possible* combinations. The logical law of large numbers thus turns out to be but a broken reed if used to support an *a priori* proof that chances mature with a high probability, and Mr. Williams's conclusion that chances do so mature must be judged as unproven.

We are now prepared to consider the second half of Mr. Williams's argument—his *a priori* justification of induction; and in the light of what has already been said, little space will be required to evaluate his demonstration. The problem to which he addresses himself is the following. In induction we are apparently faced with a situation which is the reverse of that presented by the statistical syllogism. For in the statistical syllogism we assume as a premise a proposition about the statistical composition of some class of elements, and conclude to a proposition about the composition of samples drawn from that class. In induction, on the other hand, our premises supply information about the statistical composition of samples, and our conclusion asserts something about the composition of the entire class from which the samples are drawn. Is there a *logical* ground for inductive inferences, similar to that which can be found for the statistical syllogism? This is the problem Mr. Williams believes he has solved by exhibiting as a logically necessary truth the "inductive principle": any class or population of individuals will probably and approximately match any of its samples in some specified statistical composition, provided that the samples are sufficiently large.

Mr. Williams's *a priori* proof of this principle traverses territory already familiar, for the argument is simply a restatement of Bernoulli's theorem and the statistical syllogism. The demonstration in outline runs as follows. Let M be a class (or population) of individuals, in which a proportion m/n possess some property P, where neither the size of the population nor the numerical value of this ratio is assumed to be known. Let S (the sample population, or hyperpopulation) be the set of all possible combinations (or samples) of k elements each, formed out of the population M. It then follows necessarily that if k is large enough, the overwhelm-

ing majority of samples in S have a statistical composition approximately equal to m/n. Thus far Mr. Williams is simply reporting an analytical truth of arithmetic. He concludes from it that "it is overwhelmingly probable" that any sample actually drawn from M matches the population in statistical composition, so that (since the relation of matching is a symmetrical or convertible one) it is highly probable that the population "has approximately the same composition which we may . . . discern in the sample" (p. 93). He believes he is therefore entitled to assert that

> Without knowing exactly the size and composition of the original population to begin with, we cannot calculate . . . exactly what proportion of our "hyper-marbles" [i.e., of the sample population] have the quality of nearly-matching-the-population, but we do know *a priori* that most of them have it. Before we choose one of them, it is hence very probable that the one we choose will be one of those which match or nearly match; after we have chosen one, it remains highly probable that the population closely matches the one we have, so that we need only look at the one we have to read off what the population, probably and approximately, is like. [Pp. 98–99.]

Mr. Williams terminates his discussion of the probability of induction with a bold *Quod erat demonstrandum*. Nevertheless, the demonstration involves the same fatal flaw in the interpretation of Bernoulli's theorem that has already been noticed in the discussion of the maturity of chances. Were Mr. Williams's reasoning sound, it would follow, merely on the evidence that the administration of a hitherto untested drug to a single "largish" group of sufferers from some disease had improved the condition of most of them, that it is highly probable that the administration of the drug to the entire population of sufferers from that disease would produce beneficial results in approximately the same ratio. Were his proof cogent, it would be reasonable to maintain the absence of other evidence that since a man of 65 has lived through about 24,000 revolutions of the earth around its axis, it is highly probable that he would not fail to be alive during every future revolution of the earth. To put it mildly, such consequences of Mr. Williams's thesis are not plausible, and hardly add persuasive force to his argument. He has in fact attempted to anticipate the apparent damage they do to his case by a discussion, unfortunately not altogether explicit, of the rôle of "previous inductions" in contributing to or detracting from the probability of new hypotheses (p. 117). But these supplementary remarks on the logic of science do not affect the nerve of his *a priori* demonstration of the inductive principle, and do not remedy its fatal weakness. For as has already been noted, the demonstration in order to be cogent involves the tacit but unsupported assumption that the relative frequency with which samples possessing a certain statistical compo-

sition are *actually drawn* from a population is approximately the same as the ratio of such samples in the *sample population of logically possible samples.*

Mr. Williams is occasionally aware that such an assumption is indispensable to his argument. He explicitly recognizes the "presupposition" that "any one sample . . . is antecedently as likely to be selected as any other" (p. 99); and he acknowledges that "certain kinds of samples, or samples collected according to certain prescriptions, are at any rate more likely to be representative than others" (p. 118). But he does not stop to inquire what is involved in his use of the word "likely"—the word *can not* mean what he has agreed to understand by the word "probable" without making nonsense of his statements; and he maintains that the "presupposition" just mentioned is simply an "innocent truism." That it is not a truism is recognized in recent physical theory; for the use of the so-called Bose-Einstein statistics for specifying probabilities in certain domains is in fact an explicit rejection of it. It is undoubtedly because he thus fails to appreciate the significance of this "truism" that he must confess that "If there is any puzzle about induction, . . . it is how we manage to go wrong as often as we do" (p. 167). For without the assumption, sometimes warranted by the facts and sometimes not, that a given method of sampling a population would actually select all samples of a specified size with roughly the same relative frequency, arithmetic can not assure us that we are bound to uncover more samples approximately matching the population than samples that do not.

If these comments on Mr. Williams's argument are well taken, he has produced considerably less than an *a priori* demonstration of the validity of induction. His book does possess the undoubted merit of directing attention to a problem that is important for all theories of knowledge. It is indeed questionable whether, as he believes, the spread of the conviction that there is no *a priori* foundation for induction "will slowly shatter civilized life and thought, to a degree which will make the modernist's loss of confidence in Christian supernaturalism, so often cited as the ultimate in spiritual cataclysms, seem a minor vicissitude," or whether "To dispute the rational validity of induction . . . is to deny that reason and good-will have a purchase on reality, to deny mind's hope of acclimating itself to any world whatever, natural or supernatural" (p. 16). But, in any case, the study of the principles of evidence and of the grounds upon which evidence acquires probative force, is an occupation worthy of the best efforts of philosophers.

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BOOK NOTES

Modernity and Liberty. HORACE MEYER KALLEN. (The University of Buffalo Centenary Lectures on the Problems of Freedom in the Modern World.) Buffalo, New York: The University of Buffalo Studies, Vol. 18, No. 2. March, 1947, pp. 69–130. $1.00.

For Professor Kallen, "modernity" has no reference to time; it is "a quality and form of human living far more surely than it is a date in history" (p. 75). There were "modern" men in ancient times as there are "un-moderns" living today. The essential difference between the modern and the un-modern quality of living is that the modern mind has as its vital principle "openness, mobility in all the relationships of men and ideas," whereas the un-modern mind views the world as an unchanging, closed, hierarchical order. "The un-modern mind exalts teleology over mechanism and subordinates technics to agency. It thinks experience statically, in terms of completion, not dynamically in terms of progression" (p. 76).

Having set up this distinction, Kallen inevitably sees the fulfillment of modernity in liberty, the culmination of un-modernity in authority. The freedom which is the hallmark of modernity is not merely the "freedom of the law-taker," *libertas obedientiae.* It is not merely the willing harmony of the individual with the social order of which he is a part. "The modern way makes freedom in and for the person the first and last thing; it takes all authority and all law to be secondary and derivative. It demotes them from powers that rule into instruments that serve" (p. 85). Institutions, he recognizes, are organisms; but he insists (gallantly if unfashionably) that institutions are "organizations of liberty" as are habit, law, order, and the mechanism of nature. Humanity is central; and freedom is the heart of the humaneness of humanity.

From this briefly sketched humanism, Kallen examines the organization of freedom in nature, in the democratic society—"the free society of free men," and in the extension of democracy to the making of an international mind. There is nothing here that has not been said before, and nothing that can not bear many more repetitions; surely it is as vitally important to bear testimony to man's faith in man as it is to assert man's faith in God.

If there is any point at which Kallen's thesis requires elaboration, it is in his assertion of science, industrialization, democracy, and peace as the factors which "together constitute the differentiae of the modern mind" (p. 76). Even if the togetherness of these be stressed, and the misuse of these factors when taken separately be emphasized (as Kallen does for the authoritarianisms of Nazi

Germany, Spain, Russia, Vatican City, and Nationalist China) there seems to me a danger of spurious oversimplification in the virtual equating of the democratic faith and process with the scientific spirit and method. Professor Kallen should explain in much greater detail how either the determinism of nineteenth-century science or the statistical indeterminacy of twentieth-century science can become the instrument of a freedom which is creative and not merely obedient. Further, I can not see how science can be anything but indifferent to what order of society it serves; the atom did not explode at Hiroshima because it was the instrument of a free society of free men, any more than because it was the instrument of Christian men. The atom cares nothing for either democracy or Christianity.

J. L. B.

Milton's Paradise Lost. A Commentary on the Argument. JOHN S. DIEKHOFF. New York. Columbia University Press. 1946. 161 pp. $2.00.

An examination of Milton's theory of poetry indicates his acceptance of the classical view that the chief function of poetry is to instruct. The poet is not only a maker but also a teacher. Poetry should also persuade, should move men to follow the way of life indicated in the instruction. Dr. Diekhoff has set himself to analyze *Paradise Lost* as a moral poem; to find out what instruction Milton intended the poem to convey to its readers.

Before entering into his discussion of this question, Diekhoff examines in terms of rhetorical theory the apparently arrogant self-concern of Milton's prologues, and decides that they serve the purpose of establishing Milton's claim to an authoritative right to discuss the central themes of the poem. Milton attempts also to "establish an attitude toward his subject" by careful selection of detail and choice of descriptive epithet. It is this latter rhetorical technique which provides Dr. Diekhoff with his criterion of Milton's intention in the poem.

The chief polemic of this little work is against those who, like Shelley, find enough magnificence in the figure of Satan to insist that Milton, though he might have intended to justify God, succeeded rather in glorifying Satan. Shelley's argument in his "Defence of Poetry" Diekhoff terms eloquent nonsense, and his refutation consists largely in pointing to Milton's adverse comments upon Satan. Other themes which are considered are "Man's Guilt," "God's Justice," and "God's Providence and Mercy." In these sections, Diekhoff's careful analysis reveals Milton as not par-

ticularly sound in his Puritanism. Milton appears rather as a Christian Platonist for whom reason is the highest of the virtues, and for whom God's rule is by virtue of God's merit, not arbitrary assumption.

Diekhoff's reading of Milton is on the whole an excellent conservative attempt to find out what Milton meant. It comes, therefore, as more of a shock when the author permits himself an occasional tendentious sentence which weakens his case by losing the sympathy of this reader at least. Thus "Twentieth century ignorance of God and of angels need not interfere with our acceptance of Milton's use of them. The poem may even help to dispel some of that ignorance" (p. 11). "The grandeur of Satan had led some who do not admire God into admiration of the devil" (p. 26). "Those who do not find [Satan] abhorrent have misread the poem. They will do well to ask whether their liking for Satan does not spring from enmity to God" (p. 48). "Those who find Adam right in preferring Eve and turning his back on God . . . may well ask whether they do not share in Adam's sin, preferring lesser goods to the greatest" (p. 75). "The admirers of Satan have often perhaps been those to whom the word *rebel* is not a term of reproach, but one of commendation" (p. 78).

This tendentiousness comes home to roost in the final chapter, which is a sermon with remote connections with Milton, under the title "The Way of Virtue." Here Diekhoff presents his views on the determinism of modern philosophical ethics, which somehow he finds coëxisting with a concern for freedom; not liking determinism, neither does he approve of freedom unless interpreted in his own way. So intermingled by this time are Diekhoff and Milton that it is not clear to me which speaks the epilogue:

> By persevering effort, by intelligence and self-discipline, prudence and temperance, fortitude and justice, by faith, hope and charity, we may achieve the individual and communal happiness that is the reward of virtue in men and in nations. [P. 149.]

It is a pity that these homiletic intrusions have been left to spoil a lucid exercise in literary analysis.

J. L. B.

NOTES AND NEWS

We regret to learn of the death of Dr. Edwin D. Starbuck, Professor of Philosophy and Psychology and Director of Character Education Research at the University of Southern California, on November 18, 1947, at the age of 81.

We print below the program of the Second Inter-American Congress of Philosophy to be held jointly with a General Meeting of the American Philosophical Association and the Forty-Fourth Annual Meeting of its Eastern Division, at Columbia University, New York City, December 28, 29, 30, and 31, 1947:

SUNDAY, DECEMBER 28.

4:30 P.M. Informal Tea.

8:00 P.M. Formal Opening of the Congress.

Existe una filosofía Iberoamericana?
Risieri Frondizi (Argentina)

Is There a North American Philosophy?
Ralph Barton Perry (Harvard University)

MONDAY, DECEMBER 29.

9:00 A.M. Oriental Philosophy and Religion.

(Joint Meeting with the National Association of Biblical Instructors)

El extremismo de la filosofía oriental
Vicente Fatone (Argentina)

Philosophy and Philosophers in the Far East
E. A. Burtt (Cornell University)

Discussion Panel: *Swami Nikhilananda, Mrs. Cornelia LeBoutillier, Richard Hocking, Chung-Shu Lo.*

9:00 A.M. Theory of Knowledge: Reason and Intuition.

La filosofía de las ciencias ... *Julio César Arroyave* (Colombia)

Possibilidades epistemologicas de la filosofía existencial
Humberto Piñera Llera (Cuba)

Mysticism and Semantics
Paul Henle (Northwestern University)

Discussion Panel: *Risieri Frondizi, A. E. Murphy, Virgil C. Aldrich.*

2:00 P.M. Philosophy of Religion and Spirit.

(Joint Meeting with the National Association of Biblical Instructors)

The Problem of God ..... *Clarence Finlayson* (Chile)

Sobre el espíritu y la actitud espiritual en las grandes culturas *Francisco Romero* (Argentina). (Read by *E. S. Brightman*)

Method in Philosophy and Theology
Robert L. Calhoun (Yale University)

Discussion Panel: *Marjorie S. Harris, Gregory Vlastos, Charles Hartshorne.*

7:00 P.M. Dinner of the Congress.

9:00 P.M. Smoker.

TUESDAY, DECEMBER 30.

9:00 A.M. Metaphysics: Existence.

Concept, Process, and Reality
Charles De Koninck (Université Laval, Quebec)
Ser, valor y existencia ....*Aníbal Sánchez Reulet* (Argentina)
Metaphysics and Existence
W. T. Stace (Princeton University)

Discussion Panel: *Marvin Farber, Otis Lee, Paul Weiss.*

9:00 A.M. Aesthetics and Philosophy of Art.

Desarollo estético de la creación ...*José Vasconcelos* (Mexico)
Arte y sociedad .....*Samuel Ramos* (Mexico)
Nature, Convention, and Art ...*Eurýalo Cannabrava* (Brazil)
Two Types of Aesthetic Experience, or the Art of Delight and the Art of Relief
Stephen C. Pepper (University of California at Berkeley)

Discussion Panel: *Eliseo Vivas, J. Loewenberg, Henry D. Aiken.*

2:00 P.M. Ethics and Philosophy of Law.

On a Problem of Ethics .....*Camille Lhérisson* (Haiti)
Justicia y seguridad jurídicia. Discusión de la tesis de Gustavo Radbruch .....*Eduardo García Máynez* (Mexico)
The New Subjectivism in Ethics
Brand Blanshard (Yale University)

Discussion Panel: *Juan Barona, Radoslav A. Tsanoff, Charles A. Baylis.*

7:00 P.M. Annual Banquet of the Eastern Division of the American Philosophical Association. The Presidential Address by Cornelius Krusé: "Philosophy's Contribution to World Understanding."

WEDNESDAY, DECEMBER 31.

9:00 A.M. Philosophical Anthropology.

Individualidad y comunidad .....*Eduardo Nicol* (Mexico)
Concerning Man .....*Carlos Cueto Fernandini* (Peru)
Compenetración entre la cultura Iberoamericana y la Norteamericana .....*Leopoldo Zea* (Mexico)
The Comparative Method and the Nature of Human Nature
John Allan Irving (University of Toronto)

Discussion Panel: *Miguel Jorrin, Hugh Miller, J. H. Randall, Jr.*

2:00 P.M. Philosophy of History and Culture.

La filosofía de los valores
Benigno Mantilla Pineda (Colombia)

The Philosophy of Culture in Its Bearing on the Philosophy of History .....*F. S. C. Northrop* (Yale University)

Philosophy of Democracy as a Philosophy of History
Sidney Hook (New York University)

Discussion Panel: *Patrick Romanell, Maurice Mandelbaum, Irwin Edman.*

2:00 P.M. The Philosophical Bearings of Modern Logic.

(Joint Meeting with the Association for Symbolic Logic)

E. Husserl y J. Joyce, o Teoría y práctica de la actitud fenomenológica .....*Juan David García Bacca* (Mexico)

El problema del método en la investigación filosófica
Jorge Millas (Puerto Rico)

The Relation of Logic to Metaphysics
Everett J. Nelson (University of Washington)

Some Reflections on the Theory of Systems
Nelson Goodman (University of Pennsylvania)

Discussion Panel: *Elizabeth F. Flower, Alice Ambrose, W. V. Quine.*

The annual meeting of the Charles S. Peirce Society will be held in Room 716 Philosophy Hall, Columbia University, December 28, 1947, at 2:30 p.m. Much important business must be considered, including a report and discussion on the projected volume of essays on Peirce. Inquiries may be addressed to the secretary of the Society, Frederic H. Young, 73 Orange Road, Montclair, New Jersey.

We are pleased to announce the appearance of a new philosophical quarterly entitled *The Review of Metaphysics.* Its editor is Professor Paul Weiss and its managing editor is Eli Karlin. Fr. Philotheus Boehner, Charles Hartshorne, Anton Pegis, John Wild, and F. S. C. Northrop are on its editorial board.

The table of contents of the first issue is as follows: "Dialectic and Negation" by Otis Lee; "God as Absolute, Yet Related to All" by Charles Hartshorne; "Essence, Existence and Being" by Paul Weiss; "An Ontological Examination of Causation" by Abner Shimony; Review article—*The Politics of Aristotle,* ed. by E. Barker, reviewed by Emil I. Fackenheim.

We take the liberty of quoting the following statement by the editor: "The Review of Metaphysics is an independently published philosophical quarterly which solicits the support of everyone interested in the development of a vigorous journal in America devoted to the pursuit of fundamental speculative and historical studies. It is the intention of The Review to publish technically competent contributions, whatever their source. In furthering this work the Editor will be assisted by a Board of Consulting Editors, although final responsibility for the selection of all editorial content will rest in his hands."

The address of the new review is The Review of Metaphysics, 201 Linsly Hall, Yale University, New Haven, Connecticut. The yearly subscription is $5.00, single copies, $1.50.

It must be evident to all who are acquainted with philosophical periodicals that the appearance of this new review is an event of major importance and that it presents a welcome opportunity for the publication of contributions to metaphysics. We take this occasion to congratulate the editors and to wish the *Review of Metaphysics* a long and distinguished career.

In view of Professor G. E. Moore's wish to be relieved of the editorship of *Mind* on the grounds of health, the Executive Committee of the Mind Association at a meeting held July 5, 1947, appointed Professor Gilbert Ryle, Waynflete Professor of Metaphysics in the University of Oxford, to be the Editor of *Mind* in the place of Professor Moore for the issues of the journal during the three calendar years beginning January 1, 1948.